Response to Reviewers and Editor

Before our point-to-point reply, we would like to start with some general comments:

We are aware of the circumstance that our manuscript addresses aspects subject to controversial debates in the literature, which we believe originates partially also from different languages and acknowledged methodologies in different sub-disciplines in this field. For this reason, we are not surprised by the ambivalence of the referee reports. Reviewers 1 and 2 on the one hand and reviewer 3 on the other hand reflect the spectrum of opinions we found in the literature. It is exactly this ambivalence of opinions that motivated us to write this work, and submit it to the Journal of Fluid Mechanics. We understand the point of view shared by some of the reviewers only too well, as it exactly reflects our own original perspective. But then we obtained results that were in rather severe conflict with state-of-the-art opinions from literature and caused us to rethink our perspective and investigate it deeper. Let us point out that we therefore do not want to criticize this circumstance at all, but that we would like to ask the reviewer to give the present manuscript a second thought once one has encountered that some of the conclusions of this work appear to be in conflict with parts of the literature. Of course, the literature on this controversial topic provides a spectrum of opinions rich enough to always find arguments for and against a certain point of view. We have observed heated discussions and hardened fronts in the literature, and we believe this is due to the fact that this topic requires to rethink the suitability of some numerical methods (exactly or discretely energy-preserving schemes, that have presumably developed as the gold standard in turbulence research), when it comes to investigating problems with suspected (not provable) anomalous energy dissipation.

Let us phrase a question that summarizes what we as authors identify as the central point of discussion: Taking the opinion for granted that energy-conserving discretisation schemes are to be preferred over any other numerical scheme with some form of regularization, artificial viscosity, numerical dissipation etc., then the following problem arises:

Which numerical method would one use in order to numerically investigate Onsager's conjecture on anomalous energy dissipation?

It appears to be clear that the answer can not be "a discretely energy-conserving scheme without any regularisation, artificial visosity, numerical dissipation, etc.", since this decision would already exclude that the outcome of such a numerical study is open to both a confirmation and falsification of Onsager's conjecture. In some of the reviewer reports we found that the reviewers did not comment on this fundamental aspect. In our opinion, this indicates that: (i) there is a widespread believe that new numerical methods with regularization/dissipation/etc. can not produce new physical insight, and (ii) that this reasoning justifies to not further think about the main contribution of the present manuscript but rather consider it irrelevant since those interested in the "true physics of incompressible Euler dynamics" do not need or use such a method. The latter aspect immediately redirects to the above question.
In this context, it appears to be natural to ask another question: What is the harm done by a dissipative discretisation scheme, a discretization scheme whose dissipation is confined to the smallest resolved scales and which can be shown to be consistent and to converge optimally for well-understood problems? In the present manuscript, we foster a perspective that the use of discretisation schemes with numerical dissipation enlarges rather than limits the applicability of such a scheme. As already emphasised above, a discretely energy-preserving scheme is too restrictive in our opinion since is does not provide the freedom to conduct studies where the result is open regarding Onsager's conjecture. We think the ambivalence of opinions manifested in the referee reports is actually an exclamation for a strong need to discuss the present manuscript in the scientific community. At the same time, our intention of this work is by no means to provide definitive answers to the questions of anomalous energy dissipation or finite-time singularities, but rather as an additional perspective that complements the state-of-the-art and helps to improve understanding of this difficult problem.

Let us point out that there are other works supporting the main conclusions of the present work which have not been cited in the original manuscript (since these works were under review at that time or since we were not aware of these studies at the time of writing). A recent publication on the one-dimensional Burgers equation that also addresses singular solutions and how to suppress thermalisation or white noise by means of numerical techniques in a fashion similar to what we propose for the 3d Euler equations is the following

- Sugan D. Murugan, Uriel Frisch, Sergey Nazarenko, Nicolas Besse, and Samriddhi Sankar Ray, Suppressing thermalization and constructing weak solutions in truncated inviscid equations of hydrodynamics: Lessons from the Burgers equations, Phys. Rev. Research 2, 033202, 2020.

This work was published after we had submitted our manuscript to the Journal of Fluid Mechanics. Note that the link to finite-time singularities and anomalous energy dissipation according to Onsager raised in the present contribution, which has been heavily criticized by reviewers 1 and 2, is stated very prominently in the above work by Murugan et al. (2020). We are convinced that a careful study of this work (and references mentioned therein) will again support that the present manuscript has a sound basis. The present manuscript is therefore leading-edge in our opinion given that we address the challenging three-dimensional Taylor-Green problem with immense computational requirements and unprecedented spatial resolution, while the work by Murugan et al. (2020) is restricted to an investigation of the one-dimensional Burgers equation with extrapolations to 3d Euler. Another work dealing with a simplified one-dimensional model, the nonlinear Schrödinger equation, and explaining the phenomenon of energy-dissipation through finite-time singularities is the following one

- Josserand, Christophe, Pomeau, Yves and Rica, Sergio, Finite-time localized singularities as a mechanism for turbulent dissipation. Phys. Rev. Fluids 5, 054607, 2020.

According to our understanding, our results for 3d Euler are also in line with the reasoning in the above work. Furthermore, reviewer 3 made use aware of an earlier publication by

- Cichowlas C, Bonaiti P, Debbasch F, Brachet M. Effective dissipation and turbulence in spectrally truncated Euler flows. Physical Review Letters 95(26), 2005.

This work deals with 3d Euler and exhibits interesting parallels to the present work. This study and the present one can be seen to verify each other, which is very interesting given that the numerical approach used here is fundamentally different from the one in Cichowlas et al. (2005). A strength of the present work is that our approach allows to avoid thermalisation, i.e., the solution is also meaningful in physical space. In the revised manuscript, we discuss this work in detail. Unfortunately, this work originally slipped our attention and, hence, we missed to discuss the work by Cichowlas et al. (2005) in the original manuscript. We are grateful to the reviewer for making us aware of this mistake.

Against this background, we would like to ask the reviewers to re-evaluate our work, given that we think the above works support all our main conclusions. We think that these (new) insights by no means diminish the novelty of the present contribution, but rather help in understanding its main arguments in case some of the reviewers have heard those arguments for the first time in the context of the present manuscript.

One recommendation has been that the work would be placed better in a journal with a more numerical focus. Let us briefly explain why we see strong reasons to submit our work to the Journal of Fluid Mechanics. In our opinion, recent decades of research related to the problem of finite-time singularities and anomalous energy dissipation have shown that a separation of disciplines, with fluid mechanics experts on the one hand and experts on numerical PDE solvers for the Navier–Stokes and Euler equations on the other hand, have revealed that it is difficult to make progress in this field without a substantial component of interdisciplinarity. From the above discussion and the manuscript, we conclude that state-of-the-art discretely energy-conserving schemes are not suitable to address Onsager's hypothesis numerically, as the underlying assumptions related to the design of the numerical discretization scheme already rules out the conjecture. Instead, the numerical approach chosen by our work uses dissipation mechanisms of discontinuous Galerkin schemes that are known to act on the finest resolved scales only. By carefully assessing the grid resolution over more than two orders of magnitude (32^3 to 8192^3 effective resolution), the mechanisms are shifted to increasingly fine scales and thus either point to "different" physical effects collapsing in the same global behavior of kinetic energy along all data points (in case there are no singularities) or singularities that get regularized. From the referee reports, we got the impression that this aspect has not been accepted as a viable strategy. We believe the in-depth discussions in Section 1 of the manuscript with a multitude of references to the literature (with the aim to cite contributions both for and against our own point of view in a balanced way) deserve a more reflected evaluation by the reviewers. We want to stress that many previous publications in JFM that have been cited in the present manuscript also contain a substantial numerical component, however, with the restriction that many of the numerical methods available at that time had inherent limitations, that require to rethink the whole problem from a numerical perspective in our opinion.

Response to Review #1:

The authors would like to thank the Reviewers for their valuable comments and remarks helping to improve the manuscript. Changes in the revised manuscript due to the comments of Reviewer #1 are highlighted in green, changes due to Reviewer #2 in red, changes due to Reviewer #3 in orange. Own improvements by the authors are highlighted in blue. We address the Reviewer's comments, suggestions and questions point-by-point below.

Because no direct evidence is provided, either for or against singularities of the incompressible Euler equations, the premise in the first line of the abstract of this paper is misleading and without major refocusing and re-organisation, this should not appear in the Journal of Fluid Mechanics.

We tried to improve the abstract according to the comments made by the reviewer. We understand our work as a contribution to the unsolved problem of finite-time singularities and anomalous energy dissipation. Since "misleading" is a strong accusation, we want to point to our proposal of an indirect approach for the identification of finite-time singularities. Of course, one should assess and discuss the suggested method critically, and we merely see our work as one additional data point on this topic. Yet, we find the term "misleading" inappropriate against the background that, in mathematics, an indirect proof is not questioned just because it is indirect.
In short, we argue as follows: Onsager's hypothesis says that energy dissipation in incompressible Euler flows is due to singularities. Now we observe that the kinetic energy evolution converges to a solution with non-zero kinetic energy dissipation rate. Assuming that no singularities are present, one would expect that the kinetic energy dissipation rate tends to zero under mesh refinement. However, our numerical experiments (on a very wide range of resolutions and using the largest computations of this problem known to us) show a clear trend to a non-zero kinetic energy dissipation rate. Thus, this contradiction may serve as an indirect indication of finite-time singularities. Of course, one might argue that the picture is not obvious or well-understood for the challenging 3d TGV problem, which is why we first studied the 1d Burgers example with formation of a shock and the occurrence of dissipation of energy in order to prepare this line of argumentation for more complicated problems. We have the impression that the reviewer originally did not react to this perspective or this line of argumentation.

Furthermore, their attempts to invoke arguments from Dubrulle are not relevant. A numerical study more relevant to her arguments can be found in McKeown, R., Ostilla-Monico, R., Pumir, A., Brenner, M.P., Rubinstein, S. M., Phys. Rev. Fluids, 3, 124702, A Cascade Leading to the Emergence of Small Structures in Vortex Ring Collisions, 2018. In that study a break-down of vortex sheets into smaller tertiary vortices suggests physics beneath regular Navier-Stokes. They use the standard Navier-Stokes equations, rather than a parameterised Euler as in this submission.

The authors have difficulties in understanding the reason behind the indication in the first sentence of the above comment, and would appreciate an elaboration as to what part of the reasoning should be reconsidered. We studied the reference mentioned by the reviewer and found that it does not discuss newest contributions and insights into the challenging topic of finite-time singularities and anomalous energy dissipation, contributions that have already been cited in the original manuscript (extended by most recent contributions in the revised manuscript). As pointed out by the reviewer, the work by McKeown et al. does not address inviscid flows, neither in the experimental part nor in the numerical part of that study. Nevertheless, we cited this work in a different context in Section 1.1 of the revised manuscript.
Let us further elaborate on that point: The work mentioned by the reviewer writes in Appendix D: "The incompressible Navier–Stokes equations are solved using an *energy-conserving* second-order centered-finite-difference scheme in cylindrical coordinates with fractional time stepping.", where we highlighted the term *energy-conserving*. The main point of the present work is that such discretization schemes (see Section 1 and in particular Sections 1.3 and 1.4 of the manuscript) can not be used to investigate Onsager's conjecture on anomalous energy dissipation. The work by McKeown et al. further writes "Resolution adequacy was checked by monitoring that the viscous dissipation and the energy balance are within the acceptable bound of 1%." While this procedure is generally accepted for Navier–Stokes, it is not expedient for Euler. In the Euler case, the viscous dissipation is always equal to zero. A numerical study using such an energy-conserving discretization scheme must therefore come to the conclusion that

anomalous energy dissipation can not occur. In other words, the outcome of such a study only verifies the assumption of energy-conservation that has been made prior to conducting numerical experiments. We are convinced that one needs to judge the present manuscript from all those different perspectives in order to identify its novelty. The authors made the experience that this will bring one to the point where arguments appear to be in conflict at first sight regarding this controversial topic, because many works in the literature claimed the opposite and because the use of energy-conserving schemes is so wide-spread that one typically does not question this. However, we believe that the present contribution can indeed clarify these conflicts.

What is presented, and might be publishable but in different venue, is another algorithm for doing numerical Navier-Stokes, that is Euler simulations with numerically-induced viscosity. Roughly in the same class as EULAG and a more interesting paper would be to do comparisons between their numerical Navier-Stokes and EULAG, using the examples in Drikakis and Smolarkiewicz, JCP 172, 309 (2001).

The example in Drikiakis and Smolarkiewicz is exactly the example studied in Section 3.3 of the present manuscript. As cited in the manuscript, this example dates back at least to Brown (JCP, 1995). The present manuscript explains in detail why the present work improves the state-of-the-art in terms of robust and generic numerical methods suitable to solve this problem. Against this background and after studying our manuscript and the references cited in this section, we consider this aspect mentioned by the reviewer inappropriate. While we agree that the discretization scheme does indeed exhibit numerical dissipation mechanisms, our numerical experiments with many different grid resolutions of the present discretization scheme (discontinuous Galerkin of moderately high order, polynomial degree $k = 3$) ensure that the numerical dissipation acts on increasingly finer scales. Of course, there could be effects only visible for a resolution of $100,000^3$ grid points or even more also with our approach, but we believe the trends towards convergence (with less than 1% of deviation between the two finest grid levels) is striking, which we believe is convincing enough to be published and subject to scientific debate. We consider the present numerical approach the "most consistent" with the equations at hand, as the dissipation disappears once the effects to be represented become well resolved and the jumps in the numerical solution across different elements disappear. Of course, this excludes the genuine discontinuities (shocks) that never get resolved no matter the resolution. By "most consistent", we think of a scheme whose dissipation is not primarily driven by additional parameters like in competing numerical approaches such as the techniques of artificial viscosity, filtering, or spectral vanishing viscosity frequently used in the continuous spectral element community. Apart from that, it would indeed be interesting if the topic regarding numerical dissipation in incompressible Euler flows was addressed by one of these high-order spectral element solvers as well. However, we consider such comparative studies beyond the scope of this contribution.

To understand why their work is irrelevant to understanding the regularity of Euler, authors should take a careful look at the Euler section in Brachet et al (1983), culminating in figure 12 showing the growth of the skewness. For a flow with one isolated region of growth like TG the skewness is a good proxy for the maximum of vorticity, since Beale-Kato-Majda showing the importance of $\|\omega\|_\infty$ was not published until the next year. This, plus the more recent work of Brachet and Bustamante, shows the TG Euler dynamics truly ends around $t = 4.2$. So with that understanding, what exists in the current set of calculations between $t = 4.2$ to $t = 8.6$ is just their version of numerical Navier-Stokes. A very clean version of that paradigm given their ability to reproduce the peak of dissipation in Brachet (1991).
I suggest that the authors conduct the following exercise. Take their two highest resolution calculations and identify when they begin to diverge significantly and call this t_r. Figure 9a with $\|\omega\|_\infty$ suggests $t_r \sim 4$. Then find the skewness from the higher resolution calculation, but only up until t_r, and compare with those in Brachet et al (1983). If the Euler curves match, then use the high-resolution data up to t_r to determine whether $\|\omega\|_\infty$ grows faster or slower than doubly exponential. And convince themselves that $t = 8.6$ has nothing to do with true Euler dynamics.

In our opinion, the state-of-the-art does not support the mono-causal perspective taken by the reviewer. The reviewer here mainly refers to research by Marc Brachet. However, let us share the information that Marc Brachet contacted the authors of this manuscript via email in order to discuss this work after a preprint was published on arxiv. From this email correspondence we got the impression that Marc Brachet does not seem to share the point of view of the reviewer. We therefore see the concerns raised by reviewer 1 as originating from a somewhat one-sided interpretation of previous research by Marc Brachet, which we do not find our work should get accused for. We believe this point could be easily clarified by a third opinion. For us further exchange of arguments is difficult if the reviewer is totally convinced that "TGV Euler dynamics truly ends at t=4.2" and "convince themselves that t=8.6 has nothing to do with true Euler dynamics.". The reviewer does not refer to results of the present manuscript or criticizes them. A scheme that blows up numerically does not allow conclusions regarding the occurrence of finite-time singularities. Likewise, the use of energy-conserving schemes can not be used as an argument that Euler dynamics must be energy-conserving. Likewise, we consider the reasoning inappropriate that every other scheme must be an artificial viscosity method which yields unphysical results in the inviscid limit. In the manuscript, we explain that our work does not give evidence supporting a finite-time singularity at times as early as $t = 4.2$. Let us also mention that the original works by Brachet are not that definite about the occurrence of a finite-time singularity as insinuated here by the reviewer. We are well aware of techniques to identify finite-time singularities as those mentioned above. We denote them as direct techniques and have explained these techniques in detail in Section 1 of the manuscript. The points mentioned by the reviewer are clearly state-of-the-art techniques or opinions from the literature, but it is also part of the truth that these techniques have not been able to prove the existence of finite-time singularities to date. For these reasons, the present work proposes an alternative route to the identification of finite-time singularities, which we believe might complement the discussion in the scientific community, for reasons discussed at length in the manuscript.

1. Paragraphs in section 1 are much too long. One almost a whole page.

The paragraphs are now well-separated into smaller units.

2. Figure 2 curves are too thin.

We improved the visual appearance of all figures in the manuscript.

3. It is a misconception, still promoted by some, that the Burgers equation, without a pressure term, is a good paradigm for incompressible Navier-Stokes. This should not be in this paper.

We do not argue that the 1d Burgers equation describes physics well for 3d turbulence, which has already been stated explicitly in the original manuscript. The reviewer argues that our reasoning for the 3d Euler equations has some weaknesses. Now, when taking the perspective of the reviewer, one would come to the conclusion that our scheme can not be able to reproduce the correct dissipation rate for the Burgers equation. However, the numerical results shown in this work demonstrate that the dissipation rate is predicted correctly for the 1d Burgers equation. We have the impression that the reviewer argues in one direction regarding the 1d Burgers equation, and in another direction regarding the 3d Euler equations. The reason why this example is in the present manuscript is therefore another one: The 1d Burgers equation is well-known to show a dissipation "anomaly" in the limit of vanishing viscosity, and the 3d Euler equations are suspected to show such a dissipation anomaly. Hence, there is a strong similarity between the 1d Burgers and 3d Euler equations when it comes to representing anomalous dissipation by a numerical method. We intentionally presented this sequence of examples from 1d to 3d, so that the reader can judge whether the argumentation is consistent. For further discussions, we would like to ask the reviewer to resolve or comment on this conflict in reasoning.
In the last paragraph of Section 3.2, we have summarized the motivation for considering this example, already in the original manuscript! We therefore expect the reviewer to not claim the opposite without any reference to the manuscript. In our opinion, the studies by Ray et al. (Physical Review E 84, 016301, 2011) and Murugan et al. (Physical Review Research 2, 033202, 2020) give a good reasoning why investigating the 1d Burgers equation makes sense for numerical reasons.

Response to Review #2:

The authors would like to thank the Reviewers for their valuable comments and remarks helping to improve the manuscript. Changes in the revised manuscript due to the comments of Reviewer #1 are highlighted in green, changes due to Reviewer #2 in red, changes due to Reviewer #3 in orange. Own improvements by the authors are highlighted in blue. We address the Reviewer's comments, suggestions and questions point-by-point below.

This manuscript revisits the old problem of singularities in the incompressible Euler (Navier-Stokes) equations. To study the problem, the authors study the classical Taylor-Green problem, using a novel high-order discontinuous Galerkin discretization scheme, which allows them to simulate the inviscid fluid equations way past the time at which the solution is thought to blow-up. They observe that the solution they construct evolves towards a finite dissipation rate, independently of the resolution, which is the famous energy dissipation anomaly.

With their numerical method, the authors study in fact 3 classical problems, in increasing order of complexity, namely the Burgers equations (in 1d), the 2d Navier-Stokes equations (solution with a star layer rolling-up), and finally, the 3d Navier-Stokes equations, with the initial Taylor-Green Vortex initial condition (TGV).

Major comment.

While reading the article, I got a bit lost. The authors do not seriously argue that there is a problem with the TGV problem after a finite time in the absence of viscosity. In fact, they do not even try to show pictures of the flow at time larger than $t = 4$, after the putative singularity develops.

We improved readability of the introduction. Moreover, we invested dedicated efforts into providing high-resolution visualization results for the inviscid Taylor–Green vortex problem at times larger than $t = 4$, that are shown in the revised manuscript. See also comments below for further discussions on visualization.

If a singularity develops, all they can do numerically afterwards is to study some "regularized" version of the Euler equations. Some authors the cite say very explicitly what they are doing (such as Larios et al, 2018). I would also interpret the numerical scheme used here as a generalization of the Euler equations, which introduces some form of regularization. I find the formulation of the authors very ambiguous. Insisting on the need to use schemes that do not explicitly conserve energy is a confusing way to say that they are introducing some (numerically) regularized way of studying the Euler equations. And at the end, this reads a bit like black-magic.

Apart from the fact that the reviewer uses regularization here in a very general way as an attempt to categorize the present discretization scheme, let us point out that the reviewer's main argument is essentially in line with the indirect approach for identifying finite-time singularities as proposed in this work. The argumentation is as follows: If we observe grid-convergence to a dissipative solution with non-zero kinetic energy dissipation rate (a regularized or weak solution of the Euler equations in the words of the reviewer), this serves as an indirect evidence of finite-time singularities (the term indirect is chosen in analogy to an indirect proof in mathematics). If the reviewer does not agree, we would like to ask the following question: How can the observed convergence to a dissipative solution be explained by a consistent and stable numerical discretization scheme if no singularity is present? We note that the dissipation mechanisms are shifted to increasingly fine scales and thus either point to "different" physical effects collapsing in the same global behavior of kinetic energy along all data points (in case there are no singularities) or singularities that get regularized. Further, the reviewer insinuates that the obtained weak solution of the regularized Euler equations is actually not a solution of the true Euler equations. Indeed, we do not dispute the fact that the numerical scheme with its inherent dissipation has an influence on the path taken by the numerical approximation; yet, we do not see a reasoning by the reviewer that would dispute the methodology used to come to the present conclusions. How can the reviewer justify his point of view with reference to the 1d inviscid Burgers example, which exhibits dissipation, whereas the 2D Euler case does not? Equipping the equations with an artificial viscosity term can be used to obtain the weak solution, a well accepted technique in the literature. Finally, let us point out that we do not use an artificial viscosity term for the 1d Burgers equation, and also not for the 3d Euler equations. Hence, we do not understand that the reviewer accuses the authors of an ambiguous formulation and not categorizing the present scheme as what it is. There is no need for accusations such as "Insisting on the need to use schemes that do not explicitly conserve energy is a confusing way to say that they are introducing some (numerically) regularized way of studying the Euler equations", given that the original manuscript states explicitly in Section 1.4 that the idea is "to capture the temporal evolution of the kinetic energy by numerical methods with appropriate inbuilt dissipation mechanisms". To highlight this aspect, it is also stated very prominently at the beginning of Section 2.2 in the revised manuscript. A careful study of the state-of-the-art on high-order DG and H(div)-conforming discretization methods will certainly rebut the "black-magic" claim. Hence, we believe the disagreement reflected in this comment is rather a result of different communities and the nomenclature they are used to, rather than an attempt by the authors to confuse readers.

The problem is made even worse from two remarks from the numerical aspects.

The first worry comes from magnifying the right-most panel of Fig.7b. The picture shows strange discontinuities of the field represented (the vorticity magnitude) across horizontal lines (and to a lesser extent, across vertical ones). Doing the same exercise with the solution at earlier time does not show any sign of such discontinuity. The panel shown above corresponds to $t = 4$, which is close to the putative singularity time ($t^* \sim 4.2$); but what is shown suggests that the regularization method used by the author is introducing some strange numerical instabilities, or other numerical artefacts. Which implies that the regularization they are introduced is definitely not innocuous: it does far more than introducing the "proper" dissipation where the solution becomes discontinuous (or develops a very thin sheet, as implicitly suggested when studying the Burgers example). For this reason, this worrisome numerical feature shown in the Figure calls for a serious discussion. At the minimum, I would insist to have an explanation of what happens here, and some understanding of what it does to the solution.

We intentionally decided for this way of presenting visualization results, showing both well-resolved and under-resolved scenarios. It would have been easy to show "better", i.e. somehow smoothened, visualizations, as it is actually quite often done in literature. But we don't think this is good practice to only show such results and, hence, on purpose did refrain from doing so – as experts in numerical methods can learn a lot from honest visualization results, i.e. visualizations consistent with the numerical schemes. We would like to highlight another perspective different from "a numerical scheme introducing strange numerical instabilities", namely "a numerical scheme that has to react and deal with under-resolution effects". Hence, there are many reasons why we believe the contradictions seemingly identified in the above comment by the reviewer are not expedient. The reviewer selected a picture for a 3D simulation, but the same argument could then be used to question two-dimensional results (the 2d example in Section 3.3 shows the same behavior). Note that such contradictions could easily be constructed for other examples where convergence of the numerical solution towards the exact solution can be demonstrated under mesh refinement. In fact, what the reviewer observes here is a numerical artefact of the discretization scheme. High-order continuous and discontinuous Galerkin schemes do not possess sufficient regularity at the interface between elements to expect the vorticity (obtained as a derived quantity from the velocity field) to be smooth. The study by

Guzman, Shu, Sequeira, H(div) conforming and DG methods for incompressible Euler's equations,
IMA Journal of Numerical Analysis, Volume 37, Issue 4, October 2017, Pages 17331771,
https://doi.org/10.1093/imanum/drw054

nicely demonstrates (in great detail and with many pictures) the difficulties in resolving the vorticity field for the 2d example (shear layer problem) shown in the present manuscript. Note that this publication uses discretization schemes highly regarded in the numerical and mathematical community. We did not elaborate on this in detail in the manuscript because this is state-of-the-art knowledge well-documented in the literature in our opinion. Note that we added this reference to the manuscript in the section dealing with the 2d shear layer problem.
To further illustrate the difficulties in interpreting visualizations of numerical results, consider laminar Poiseuille flow in a channel with a parabolic velocity field, solved numerically by a continuous finite element method with linear shape functions: The vorticity field obtained from the velocity (solved for as a

primary variable) will be piecewise constant but discontinuous. Increasing the resolution will still show discontinuities in the vorticity field. Hence, this seemingly unphysical behavior in visualizations cannot be taken as evidence for identifying severe problems of discretization schemes in general. The solution to this dilemma is to resort to more abstract concepts such as the notion of convergence, where a data point is subject to an error but with a path towards a limit value. Our point here is that we strongly argue for a more holistic view that unites physical and numerical perspectives. Requiring the solution and derivatives of it to be infinitely smooth for a finite spatial resolution simulation cannot be obtained, and is in fact against what our numerical results indicate. If singularities occur, one will always reach the point where the discretization scheme operates at its limits of resolution, where visualization results may always be termed "unphysical" due to limited regularity of the discrete solution locally.
Another main aspect highlighted in the manuscript is that one should not mix up physical and numerical blow-up. We strongly argue against using visualizations of a numerical simulation blowing up at some time as a means to support claims on finite-time singularities. We instead argue that a numerically robust and stable scheme that does not blow up is a necessary prerequisite to address the problem of finite-time singularities by numerical techniques. Every researcher developing numerical discretization schemes has seen plenty of pictures of numerical blow-up, 40 years of research on finite-time singularities have not been able to provide this seemingly intuitive and simple way of evidence for finite-time singularities. In fact, a certain amount of "trust" in numerical discretization methods is required, and for researchers new to numerical methods or discontinuous Galerkin the present work can and should not serve as the only reference. Profound knowledge about numerical methods is required by physicists to investigate such problems numerically and interpret numerical results. The problem with physical intuition becomes evident when recalling that discontinuous Galerkin methods can per se neither be considered "unphysical" due to jumps between elements (based on the argument that a quantity such as the velocity or density may not have discontinuities for incompressible flows from a physical perspective) nor "physical" when applied to problems with shocks such as in compressible gas dynamcis (based on the argument that there is a natural match regarding discontinuities between physics and numerics). If one decides for a numerical discretization scheme that is discretely energy-conserving without any dissipation mechanism in order to prove that Onsager's conjecture on anomalous energy dissipation is incorrect, that conclusion has already been built into the scheme. Instead, we try to rely on purely numerical mechanisms, in that dissipation is by construction confined to the finest resolved scales of the simulation. By increasing the numerical resolution, that mechanism transitions to different scales, yet the overall quantities observed in our experiments such as the global dissipation rate are indeed converging. We selected the three examples from 1d, 2d, to 3d examples to demonstrate that numerical methods can be powerful along the lines of convergence, especially for the problems discussed in this work that require good robustness and stability properties of a numerical scheme.

The second worry comes from the spectra shown in Fig.10. The $k^{-5/3}$ regime, which people would expect at the peak dissipation, does not come out very convincingly, to say the very least. At the highest resolution, Fig 10e, the spectrum seems to develop two regions with different spectral powers, one possibly like $k^{-7/3}$ and one actually much shallower, close to, or perhaps even less than k^{-1}. Fig.10 shows no trace of a $k^{-5/3}$ regime, by any stretch of the imagination.

There is evidence in the literature that a $k^{-5/3}$ decay of the energy spectrum in the inertial range can actually not be expected at the peak of dissipation around $t = 9$ for the TGV. In the manuscript, we have referred to the PhD Thesis by Marian Piatkowski in this context, where it is shown that the spectrum is described better by $k^{-7/3}$ at that instant of time ($t = 8-9$), but that a $k^{-5/3}$ decay is indeed observed at later times. That work also compares to results from the high-order CFD workshop to verify their results. We observed the same behavior for viscous TGV simulations at $\mathrm{Re} = 1600$, for which grid converged results can be obtained for the whole energy spectrum including the smallest scales. Hence, there is actually no reason to question the present results after a careful look at the state-of-the-art. Also the second concern mentioned by the reviewer has already been discussed in the original manuscript. It is again a property of the high-order discretization scheme used here. This phenomenon is known as energy-bump in the numerically oriented literature. It does not make sense, though, to interpret this part of the spectrum as k^{-1}. For high polynomial degrees for which the energy-bump is more and more pronounced the slope could even become positive before or around the resolution limit of the discretization, indicated by the 1%-rule in the manuscript. Again, our argumentation is that this is a numerical artefact that one has to live with when looking at the current state-of-the-art, or try to improve on that aspect. We refer to the DG literature in this context where these aspects are discussed in great detail, and references can be found in the manuscript. We already highlighted that "improving" the energy-bump behavior by some techniques suggested in the literature is a delicate issue and should be evaluated with great care.

These two comments are an invitation to think about what the numerical scheme does to the solution, before drawing any conclusion on the Onsager conjecture or anything else.

As discussed at length in the reply to the two previous comments, we disagree with the reviewer on this topic but believe that our results are obtained in a scientifically correct way, with state-of-the-art ingredients, and proposing them as a complement to existing tools, making them worth to be published for further discussion (including the possibility of falsification) by the scientific community.

From a theoretical point of view, the question of determining what is the correct weak solution of the Euler equation, corresponding the limiting case of the Navier-Stokes equations when viscosity goes to zero, is most certainly a very interesting one. I understand this work as an attempt to construct some sort of limiting solution to the Navier-Stokes problem in the zero-viscosity limit. But given the numerical evidence shown by the authors, the constructed solution appears as arbitrary. As far as I can see, there is no guarantee that the present work provides the correct weak solution of the Navier-Stokes equations in the $\nu \to 0$ limit.

To the best of our knowledge, the present work is the first that demonstrates convergence to a weak, dissipative solution of the incompressible Euler equations (see Figure 13 in the revised manuscript on error convergence), with all the open questions and controversial debates discussed in the manuscript. The fact that the reviewer does also not know what the correct weak solution is and that the reviewer can not refer to other literature presenting any (converged) weak solution clearly point to a major novelty peculiar to the present contribution in our opinion.

For this reason, I am certainly not inclined to support publication of this piece of work, at least in its present form. A very significantly revised version, discussing more clearly the limitations of the numerical scheme, and providing some better understanding of the obtained solution would need to be provided. The claims about the Onsagers conjectures appear to me as wishful thinking, and in my view need to be considerably toned down.

The "limitations" of the present discretization scheme have already been quantified in the original manuscript: (i) The section on the 2d shear layer problem points out to which extent the present (stabilized) DG discretization extends robustness and the range of applicability of the present discretization scheme compared to state-of-the-art methods from the literature. (ii) The figures showing energy spectra for the 3D TGV problem also plot the resolution limit according to the 1% rule by Moura et al. and the Nyquist wave number. These results demonstrate that our scheme performs in agreement with the resolution **limits** for high-order DG schemes known from the literature. (iii) The quantitative convergence results with L^2-errors for the temporal evolution of the kinetic energy and kinetic energy dissipation rate versus the number of unknowns of the discretization scheme transparently document the resolution **capabilities** or **limits** of the present discretization scheme in our opinion. As pointed out in the summary, it should be the subject of future studies to compare against these results and, potentially, find improved numerical discretization schemes.

Extra minor comments:

1. Concerning the Burgers equations, the authors mention that their results are in agreement with standard expectations. One notices in Fig.2 the appearance of oscillations near the region where the solution becomes discontinuous, which I would interpret as resulting from – presumably unavoidable – Gibbs oscillations near the "shock". The authors nonetheless claim that they can reduce their influence by improving the approximation scheme in their solutions. It would be good to show evidence for this.

As already explained above, the numerical solution can not be expected to be arbitrarily accurate for arbitrarily coarse spatial resolutions. An improvement in accuracy is demonstrated by numerical convergence to the correct dissipation rate as shown in the manuscript. The literature on discretization methods for shock-capturing is vast. We suggest the textbook by Hesthaven and Warburton (Springer, 2007) as a starting point in the context of discontinuous Galerkin methods (see for example Figure 5.11 in this textbook for the 1d Burgers equation and Figures 5.14, 5.15 for the 1d Euler equations, where the use of limiters is illustrated for problems with shocks). As emphasized in the manuscript, the present work does not use specialized shock-capturing techniques for the 1d Burgers equation. We actually believe that it is even more striking to see that a discretization scheme without special measures in this direction is able to correctly predict the dissipation rate. A look at the publication by Murugan et al. (Phys. Rev. Research 2, 033202, 2020) might underline that the present discretization scheme does a good job compared to other methods published nowadays, albeit the present scheme might of course not be optimal. Finally, let us note that the theory on anomalous energy dissipation for incompressible Euler flows (Onsager and Hölder regularity) does not expect singularities in the form of jumps, but rather continuous solutions of infinite gradient in the 3d Euler case.

2. Concerning the 2d Navier-Stokes calculations, it is certainly reassuring to see that energy is conserved, even when the solution generates some very small scales. I would point out, however, that in 2d flows at high Reynolds numbers, the dissipation of energy is not so much due to viscosity, but rather via some friction acting at the largest scales of the flow. This is a consequence of the upscale nature of the energy cascade (a consequence of conservation of energy and enstrophy in the inviscid limit). Perhaps an interesting question to study numerically is what happens to enstrophy, which should be constant for the Euler equations, but is dissipated by viscosity finite, but large Reynolds number (the cascade of enstrophy is downscale). I am not aware of any "enstrophy dissipation anomaly", but it may be interesting to look at the problem as the authors have done in 3d for the TVG.

We acknowledge this hint as an interesting idea for future considerations. We believe, though, that investigations in this direction would go beyond the scope of this work.

Response to Review #3:

The authors would like to thank the Reviewers for their valuable comments and remarks helping to improve the manuscript. Changes in the revised manuscript due to the comments of Reviewer #1 are highlighted in green, changes due to Reviewer #2 in red, changes due to Reviewer #3 in orange. Own improvements by the authors are highlighted in blue. We address the Reviewer's comments, suggestions and questions point-by-point below.

*This paper addresses the problem of anomalous energy dissipation and singularities in 3D incompressible Euler flows by using high-resolution numerical simulations of the inviscid TaylorGreen vortex problem. The idea is to use a novel high-order discontinuous Galerkin discretization approach *beyond* the time at which finite-time singularities have been suspected. The corresponding simulations are used to investigate whether the numerical results match the expected physical behavior obtained from classical cascade pictures in case of inviscid flows.*

The novel high-order Galerkin approach if first tested on simple problems. It is shown to perform well on a singular 1D problem: shock formation in the inviscid Burgers equation's (Figures 2, 3 and 4) and on a regular 2D problem: vortex sheet roll-up (Figures 5 and 6) in the 2D incompressible Euler equation.

*The main finding is that the kinetic energy evolution of the inviscid Taylor-Green vortex does not tend towards exact energy conservation for increasing spatial resolution of the numerical scheme, but instead converges to a solution with nonzero kinetic energy dissipation rate (see Figure 8 and Figure 1). Thus the *indirect* approach of the paper provides indications of finite-time singularities by observing the anomalous dissipative behavior in the kinetic energy evolution.*

We interpret the reviewers comments as a general consent to the main line of argumentation of the present manuscript. We further thank the reviewer for all the constructive and helpful comments provided below.

The paper is interesting and well written; however, I feel that some references about the subject of Galerkin truncation are missing in the discussions of energy conservation and time-reversibility, in section 1.3.

In particular, the following reference should be considered: Cichowlas C, Bonaiti P, Debbasch F, Brachet M. Effective dissipation and turbulence in spectrally truncated Euler flows. Physical Review Letters 2005;95(26).

*In this work (let's call it C05), the dealiasing technique does not introduce any numerical dissipation as, when properly dealiased, pseudospectra methods are numerically identical to standard (Galerkin-truncated) spectral method. The spectrally truncated equations thus exactly conserve the energy. Although the *total* energy is exactly conserved in C05, there are some analogies with the present paper where the numerical scheme directly computes/simulates the dissipation rate. In C05 the dissipation is *estimated* from the small-scale thermalized energy. The onset of dissipation around t=5-6 and the dissipation maximum around t = 8 appear to be very similar in both studies.*

I think that both studies complement and verify each other in some sense, especially since the underlying numerical methods and analysis tools are different. I thus feel that a discussion of this point should be added to the paper.

We thank the reviewer for the suggestion which we were happy to pick up. We discuss the study mentioned by the reviewer in detail in Section 1.3 of the revised manuscript.

Minor points: Other references that the authors might find useful are

For the Burgers problem: Samriddhi Sankar Ray, Uriel Frisch, Sergei Nazarenko, and Takeshi Matsumoto, Resonance phenomenon for the Galerkin-truncated Burgers and Euler equations, Phys. Rev. E 84, 016301 (2011).

The study by Ray et al. (2011) is now cited in Section 1.3 and Section 3.2 of the revised manuscript.

For the shear layer problem: Thalabard, S., Bec, J., Mailybaev, A. A. From the butterfly effect to spontaneous stochasticity in singular shear flows. Commun Phys 3, 122 (2020).

The study by Thalabard et al. (2020) is now cited in the revised manuscript in the Section on the two-dimensional shear layer problem.